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IZA DP No. 11168

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ABSTRACT

Government Programs Can Improve Local Labor Markets, But Do They? A Re-Analysis of Ham, Swenson, Imrohoroğlu, and Song (2011)*

Research on the effects of enterprise zones – especially state programs – has generally failed to find evidence of beneficial effects such job growth or poverty reduction. In contrast, Ham, Swenson, Imrohoroğlu, and Song (2011, hereafter HSIS) present evidence that state and federal enterprise zones (EZs) established in the 1990s substantially reduced poverty. However, their estimates of the effects of EZs in reducing poverty are badly overstated for two reasons. First, HSIS have a substantial error in their data on poverty rates by Census tract, which accounts for most of the estimated impact of state EZs that they find. Second, their estimates of the effects of federal Empowerment Zones (EMPZs) and Enterprise Communities (ENTCs) appear to be strongly influenced by selection of areas that experienced negative shocks. An estimator based on comparing federally designated zones to more-comparable areas that applied for and were rejected as zones, or became zones in the future, yields much smaller estimates than those in HSIS. And the large poverty-reduction effects of ENTCs that HSIS found are largely spurious – not surprisingly, given that ENTCs received meager benefits and had no hiring credits.

JEL Classification: J23, J38, R12

Keywords: enterprise zones, poverty

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* We are grateful to the Laura and John Arnold Foundation for support for this research, through grants to the Economic Self-Sufficiency Policy Research Institute (ESSPRI) at UCI. This paper is part of a larger project on the longer-term effects of enterprise zone programs. Any opinions or conclusions expressed are the authors' alone and do not necessarily reflect those of the Laura and John Arnold Foundation. We are grateful to John Ham, Charles Swenson, Ayşe Imrohoroğlu, and Heonjae Song for providing data and some of the code from their original paper. We are also grateful to Matthew Freedman, Pat Kline, and Shawn Rohlin for helpful comments.

I. Introduction

Research on the effects of enterprise zones – especially state programs – has generally failed to find evidence of beneficial effects on outcomes such as job growth or poverty reduction. However, in a challenge to this near-consensus, in a paper published in the *Journal of Public Economics* in 2011, Ham, Swenson, Imrohoroğlu, and Song (hereafter HSIS) present evidence that state and federal enterprise zones (EZs) established in the 1990s significantly improved local labor markets, with some evidence of declines in the unemployment rate, and increases in the fraction of households with wage and salary income, and in average income and employment. Their most notable and striking findings concerned the effects of EZs on poverty. Pooling across state EZ programs, they found that establishing an EZ in a Census tract reduces the poverty rate by 6.1 percentage points (HSIS Table 2). State-specific estimates ranged up to 14 percentage point reductions (HSIS Table 3), and their IV estimate (pooling across states) indicated a 26.1 percentage point reduction (HSIS Table 4).[1] Their estimated effects of federal Empowerment Zones (EMPZs) and Enterprise Communities (ENTCs) are also large; their estimated effect of EMPZs is an 8.8 percentage point reduction in the poverty rate (HSIS Table 12), and their estimated effect of ENTC designation is a 20.3 percentage point reduction (HSIS Table 16).[2]

In a recently-published survey of place-based policies, Neumark and Simpson (2015) suggested that these estimates were "implausibly large" (p. 1240). HSIS's estimated effects on poverty are certainly outliers relative to the literature, which fails to find positive effects of enterprise zone designation on poverty rates (e.g., Freedman, 2013; Hanson, 2009; Reynolds and Rohlin, 2015), although Busso et al. (2013) report sizable positive effects of federal EMPZs on other labor market outcomes.[3]

Given that different empirical approaches can yield different estimates, it is not the finding of poverty reductions, per se, that seems implausible. Rather, it is the large magnitudes of the estimated reductions. As documented below, poverty rates in areas that states designated as EZs averaged around

[1] Alternative estimates excluding the nearest non-designated zone to each zone are similar (HSIS Tables 8 and 9).
[2] The estimates are similar using IV, and excluding the nearest non-designated zone to each zone (HSIS Tables 17 and 19).
[3] See the summary table (Table 18.2) in Neumark and Simpson (2015). In a recent paper, Hanson and Rohlin (n.d.) compute a number of alternative program evaluation estimators for the effects of federal EMPZs on employment and the number of firms. They report a range of estimates, many near zero, especially in the longer-term.

18%, and poverty rates in areas designated as federal EMPZs or ENTCs averaged around 48% and 40%, respectively. Thus, the state-level estimates in HSIS imply poverty reductions of between about 33% and over 100%, and their ENTC estimate implies a poverty reduction of about 50%; their EMPZ estimate implies a somewhat smaller poverty reduction of around 18%.

We wish that HSIS had identified the magic bullet (and data, and estimator) for substantially reducing or even eliminating urban poverty in the United States. In fact, however, their estimates of the effects of EZs in reducing poverty are badly overstated, for two reasons.

First, and most fundamentally, HSIS have a substantial error in their data on poverty rates by Census tract. This accounts for most of the estimated impact of state EZs that they find.

Second, while this error is present in their analysis of federal zones (EMPZs and ENTCs), it has less of an impact on the estimated impacts of these zones. However, the data on federal zones suggest strong selection of areas that experienced negative shocks for EMPZ or ENTC designation, which could explain the large estimates of the poverty-reduction effects of federal zones that HSIS find. For EMPZs, which received substantial hiring credits and other benefits, an estimator that instead compares federally designated zones to more-comparable areas that applied for and were rejected as zones, or became zones in the future, yields much smaller estimates than those in HSIS – although the estimates still indicate that EMPZs reduce poverty. However, the strong poverty-reduction effects of ENTCs that HSIS found appear to be largely or completely spurious. This is perhaps not surprising, given that Busso et al. (2013) treated ENTCs – which received meager benefits and had no hiring credits – as controls for EMPZs – which received far greater benefits including generous hiring credits.[4]

II. HSIS's data, methods, and results

Data

HSIS use Census tract-level data from different sources (see their on-line Appendix A).[5] Their data for

[4] This comment is an offshoot of a larger project focused on estimating longer-run effects of state and federal enterprise zones. It was only when we discovered the data error in HSIS that we were prompted to write this comment, to set the record straight on the shorter-run effects they estimate before moving on to longer-run effects.
[5] This is an unpublished appendix to their paper, referenced in HSIS (2011), for which the url cited in the published paper no longer exists.

1980 come from the historical data archive at the Center for International Earth Science Information Network (CIESIN) at Columbia University. Their 1990 data are from Applied Geographic Solutions (AGS) in Thousand Oaks, CA.[6] And their 2000 Census data are from the SF-3 file from the U.S. Census Bureau.[7]

Methods

Their key results are based on a triple-difference (DDD) estimator. They study zones created between 1990 and 2000. They compute the difference in outcomes between 1990 and 2000 for tracts where zones were created, which we can denote $\Delta Y_{cs1}{}^{T}$, where Y is the dependent variable of interest, the c and s subscripts denote Census tracts and years, the *1* subscript denotes that the difference is for the post-treatment period, and the T superscript denotes that this difference is computed for treated tracts. They subtract from this the pre-treatment difference – between 1980 and 1990 – for the same tracts, $\Delta Y_{cs0}{}^{T}$.

HSIS use three different control tracts: the nearest tract, the average over all contiguous tracts, and then simply all potential control tracts in the state that are also not designated as federal EMPZs or ENTCs. The estimation strategy diverges depending on which control tracts are used. For the nearest and contiguous tracts, there is a control "tract" matched to each treated tract – a single tract in the case of the nearest tract controls, and an average tract in the case of the contiguous tract controls (averaging across the set of contiguous tracts). In these two approaches, they construct a difference-in-difference for the control tract matched to each treated tract c, $\Delta Y_{cs1}{}^{C} - \Delta Y_{cs0}{}^{C}$. To estimate a common effect of state EZs using these two types of controls (and similarly when they estimate the effects of federal zones), they form the triple-difference (DDD) as the difference between the two double-differences, and estimate a simple regression of this DDD on an intercept (using random county effects), as in:

$$\{\Delta Y_{cs1}{}^{T} - \Delta Y_{cs0}{}^{T}\} - \{\Delta Y_{cs1}{}^{C} - \Delta Y_{cs0}{}^{C}\} = \beta + \varepsilon_{cs} \quad (1)$$.[8]

This DDD estimator identifies the effects of EZ designation from the change in the dependent variable from 1990 to 2000 relative to the change from 1980 to 1990 in treated tracts, relative to the same

[6] AGS was subsequently changed to CIESIN.

[7] Ayse Imrohoroğlu provides data on her website for the purposes of replicating HSIS's analysis. See http://www-bcf.usc.edu/~aimrohor/links.htm (viewed October 19, 2017).

[8] The notation is different from that in HSIS.

difference-in-difference in control tracts.

The third approach, using all non-EZ tracts as controls, is slightly different, but conceptually the same. Here, they simply form the double-difference for every tract and estimate a regression with an EZ dummy variable equal to one for the designated tracts. Letting EZ_{cs1} denote a dummy variable for tracts designated as zones between 1990 and 2000, the model is just:

$$\{\Delta Y_{cs1} - \Delta Y_{cs0}\} = \alpha + \beta EZ_{cs1} + \varepsilon_{cs} \quad . \tag{2}$$

This is still a DDD estimator, but now the number of observations is the number of tracts, rather than the number of designated EZs.[9] When HSIS estimate equation (2) for the "all" analysis, they include state dummy variables, which essentially treats all treated and control tracts in a state as a matched pair. Note that HSIS could have implemented the estimators with the nearest or the (average of) contiguous tracts as controls using equation (2). There would then be double the number of observations, and one could introduce a fixed effect for each pair of treated and matched control tracts. However, these fixed effects drop out of the triple-difference in equation (1). Both equations (1) and (2) are easily adapted to estimate effects of separate state EZ programs: in equation (1) by adding state dummy variables (which capture the effects for each state), and in equation (2) by adding interactions between *EZ* and state dummy variables.

Using all tracts in the state as controls would be expected to provide more precise estimates, although with the potential for more bias if these controls are less similar to the treated tracts than are nearby tracts. HSIS use Hausman tests to pick which estimator to use, based on the idea that the less restrictive control tracts are efficient, but potentially biased if the assumptions underlying the validity of the control tracts do not hold for the less restrictive sets of tracts.[10]

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[9] HSIS also consider using the second-nearest tract to each zone designated as an EZ, to avoid bias from spillovers. However, they find little evidence of spillovers or of substantial differences in results, so we ignore this alternative strategy in this comment.

[10] Formally, the assumption is that the treatment and control tracts share the same quadratic and higher-order trends, and the same double-difference in any explanatory variables. HSIS use the random effects estimator in computing the Hausman tests to select the preferred estimator. In addition, when the estimator that is supposed to be more efficient (but is potentially more biased) has a larger standard error (which can happen in finite samples, even under the null hypothesis), they simply reject that estimator, since it has a greater risk of introducing bias. To be clear, though, none of the issues raised in our critique pertain to these econometric details.

HSIS also use an instrumental variables (IV) estimator, which can, in principle, correct for regression to the mean in designated zones. This can arise if zones are designated as a consequence of an adverse shock, in which case regression to the mean can generate spurious evidence of positive effects of zone designation. In the IV approach, they always use the framework in equation (2), creating a data set including both treated and untreated tracts, with an EZ dummy variable that equals one for the treated tracts. When they use the nearest or contiguous tracts as controls, they include dummy variables for matched treatment and control pairs, and when they use all tracts as controls they include state dummy variables.[11]

For each outcome on which they present evidence (unemployment rate, poverty rate, fraction of households with wage and salary income, average wage and salary income, and employment), HSIS instrument EZ in equation (2) using the value of other outcomes in 1980. (Recall that EZ is defined as one or zero for the double-differenced observations in equation (2), but designation actually occurs between 1990 and 2000.) The exclusion restriction is that these IVs are orthogonal to the residual of equation (2), so, in particular, in the model for the poverty rate, the assumption is that the unemployment rate, fraction of households with income, average income, and employment in 1980 are uncorrelated with the transitory shock to poverty (transitory, because the model includes fixed effects). This is not a very compelling assumption, given that the 1980 outcome enters the residual (since the dependent variable is the double-difference defined over 1980, 1990, and 2000). Why, for example, would transitory shocks to any of these five outcomes not be correlated with transitory shocks to the others?[12] Indeed, HSIS do not offer *any* argument as to why their instrument should be valid; they simply state the conditions under which it could be (p. 786).

[11] Instead of rerunning Hausman tests to select controls for the IV analysis, they always use the same control tracts as determined by Hausman tests in the OLS analysis.

[12] Put differently, in thinking about the IV estimator for the employment equation, for example, it seems hard to rationalize why awarding an EZ to an area depends on the past unemployment rate but does not depend on the past level of employment; the same argument casts serious doubt on the exclusion restriction for IV estimation of their model for each of the five outcomes.

III. Results and Data Issues

HSIS results for state EZs

We set the stage by briefly discussing the basic HSIS results for state EZs. Table 1, in the columns labeled HSIS, reports the descriptive statistics for 1980, 1990, and 2000, for the zones, and the three alternative sets of controls (non-EZs are denoted "NENTZ"). Note that there are some shaded rectangles, indicating cells where there were slight discrepancies between what was reported in HSIS and what we calculated using their data and code. These are minor and may just reflect transcription errors in their tables. We report the estimates computed with their data and code first (and the different estimates in their tables below, in square brackets), and rely on our replications in what follows.

In Table 2, in the columns labeled HSIS, we report the double-differences (DDs), and then the triple-differences, which are their estimated effects of state EZs. As the triple-difference panel indicates, HSIS find that state EZs, on average, lower unemployment rates by around 1.6 percentage points, raise employment (levels) and average incomes slightly, and reduce poverty by 6.1 percentage points. It is this latter estimate – and other estimates for the effects of state and federal EZs on poverty discussed below – with which we take issue.

The error in measuring poverty in the HSIS data

One can track back through the DD estimates in Table 2, and the means in Table 1, to see what drives the estimated effects on poverty in HSIS. In particular, the top (double-difference) panel of Table 2 shows that, in the HSIS data, the DD estimates for the poverty rate show a sharp increase in poverty in the EZs relative to the controls from 1980 to 1990 (the Δ90 terms), and a sharp reduction in poverty in the EZs relative to the controls from 1990 to 2000 (the Δ00 terms). And Table 1 shows that this is driven, as it has to be, by a spike in measured poverty in 1990 in the EZs – from 16.41% in 1980, to 25.67% in 1990, and back down to 17.95% in 2000. Poverty also increased in 1990 for the control zones – for example, from 11.81% in 1980 to 16.13% in 1990 and back to 12.22% in 2000 for the nearest non-EZ tracts – but not by as much. These increases in poverty in 1990 are puzzling, since there was no similar increase in poverty measured at more aggregate levels. For example, there was no uptick in national poverty in 1990 (measured on a per person basis, the same as in Table 1).[13]

To examine tract-level measures of poverty further, we use data from the Neighborhood Change Database (NCDB). A difficulty in constructing tract-level observations over time using decennial Census data is that many Census tract boundaries change over time, depending on population change. The NCDB provides consistent tract definitions over time.[14] The columns labeled NCDB in Table 1 show what should be the same measures for the EZ and control tracts as in HSIS's data.

The one immediately striking difference is that in the NCDB data there is no uptick in poverty rates in the 1990 data for either EZ tracts or control tracts. For example, in the top panel, the NCDB poverty rates for 1980, 1990, and 2000 are 16.78%, 17.79%, and 18.33%, compared to 16.41%, 25.67%, and 17.95% in the HSIS data. That is, the 1990 poverty rates for the treatment tracts in HSIS's data appear to be overstated by about 8 percentage points on average. The data for the control tracts also show a spike in poverty rates in 1990 only in the HSIS data, although the discrepancy is not quite as large. For example, for the nearest non-EZ tracts, the average poverty rate in 1990 appears to be overstated by 4.75 percentage points.

Given that HSIS's DDD estimate identifies the effect from the comparison, between EZ and control tracts, of the difference between the 1990 to 2000 change and the 1980 to 1990 change, it is clear that the overstatement of poverty in 1990 in their data – and in particular the *greater* overstatement in the EZ tracts – will generate spurious evidence of sharp declines in poverty resulting from EZ designation. This is confirmed in the columns labeled NCDB in Table 2, where we see that the estimated effect of EZs on the poverty rate is a 1.25 percentage point decline, rather than a 6.10 percentage point decline. Note that the HSIS estimate is well beyond the lower end of the 95% (and 99%) confidence interval using the NCDB data (−1.88 for the 95% confidence interval, and −2.06 for the 99% confidence interval, compared to the

[13] For example, the individual poverty rate was 13.0% in 1980, 13.5% in 1990, and 11.3% in 2000. See https://www.census.gov/data/tables/time-series/demo/income-poverty/historical-poverty-people.html, Table 2 (viewed August 4, 2017).

[14] See http://www.geolytics.com/pdf/NCDB-LF-Data-Users-Guide.pdf (viewed October 27, 2017). As discussed below, HSIS did not use the NCDB data, and matched tracts over time using other means. This is explained in their on-line Appendix A, although not in much detail.

HSIS estimate of −6.10).[15]

If the error in measuring poverty in HSIS's 1990 data is similar across states, we would expect similar bias in state-specific estimates. Indeed, the problems with HSIS's estimation of poverty rates is similar across all 13 states used in their analysis. Moreover, this error is not simply a reflection of what is in the CIESIN data (versus the NCDB). This is clear from the columns labeled "Poverty rate" in Table 3 (columns 5-6 and 11-12). Here we show, for all states and then for each state, the 1990 poverty rates in HSIS's data, as well as the 1990 poverty rates that we constructed from the same CIESIN data source that HSIS used. Table 3 shows that HSIS's poverty rates in 1990 are substantially higher than the rates in the CIESIN data. We show below that the same is true when comparing HSIS's state-level poverty rates to those in the NCDB.

Table 4 shows that the incorrect 1990 poverty measures in HSIS's data lead, in the state-level analysis, to sharp overestimation of the effects of state EZs in reducing poverty. For example, the estimate for California using HSIS's data shows a 7.14 percentage point reduction in poverty. But using the NCDB data, this declines to a statistically insignificant 2.31 percentage point reduction. As another example, the estimate for Massachusetts declines from a 13.95 percentage point reduction to a 2.07 percentage point reduction.[16]

Given the stark differences in poverty results between the HSIS data and the NCDB data (as well as the currently-posted CIESIN data), it is important to determine whether the HSIS data are clearly incorrect or instead – for some reason – provide different, but not necessarily invalid measurements. And as the results above make clear, it is the measurement of poverty at the Census-tract level in 1990 that is key.

It is straightforward to show that HSIS mismeasured 1990 poverty rates at the Census tract level. The data that HSIS make available include counts above and below poverty. We can replicate HSIS's published 1990 poverty rates using the ratios of their counts. However, as we show in the other columns of

[15] This is true for many of the estimates reported below; we do not repeat the calculation, but the reader can easily do so.

[16] The estimate for Oregon falls by nearly 40%, but still remains quite large.

Table 3, the counts of persons above and below poverty in HSIS are consistently and substantially (with a few exceptions, discussed later) *lower* than the counts in the CIESIN data – the data source that HSIS say they use. (These differences lead to the result already noted – that the poverty rates in 1990 in HSIS's data are persistently higher.)

To make the comparison with the NCDB as clear as possible, we first identified Census tracts in the NCDB that did not change over the period 1980-2000. We then merged these tracts with the HSIS data to obtain a set of tracts that are consistent over time across the two data sources.[17]

In order to replicate HSIS's data as closely as possible, we followed the procedures described in HSIS's Appendix A1 and downloaded 1980 and 1990 Census data from CIESIN.[18] In the data we downloaded, most tracts have multiple observations per tract for Census Places, Census Block Groups, etc. We aggregated these data to the Census tract level before matching it to the NCDB and HSIS data.[19] In the first four columns of Table 5, we compare the CIESIN data we downloaded with the NCDB data. The top panel is for all 13 states, and the first five rows show results for the five outcomes studied by HSIS. These results show that the CIESIN and the NCDB match up nearly exactly for all outcomes in 1990, suggesting that there were no issues in cleaning and defining variables with the two datasets. The last two columns add the estimates from the HSIS data, and show that the poverty rate is sharply overestimated in the HSIS data.

Echoing Table 3, but now for a consistent subset of tracts, it is clear that there is an error in the HSIS data. In particular, the average tract-level counts of persons above and below poverty, which are presented below the dashed line in the top panel, are much too low in HSIS's data. Whereas these numbers are, respectively, 3,338 and 498.0 in the CIESIN and NCDB data, they are lower by a factor of 10 or so in

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[17] This reduces the number of Census tracts in the HSIS data by about 50%, which is similar to the fraction of Census tracts that do not change over the same time period, according to NCDB documentation.

[18] The data were accessed from http://www.ciesin.columbia.edu/repository/ftpsite/pub/census/usa/. Files were downloaded for each state and year individually and then appended together.

[19] In HSIS' Appendix 1 they say "Census tractlevel [sic] records were identified and extracted based on a SUMMARY LEVEL value of `14' (Census Tracts/BNAs) for the required fields." When we downloaded data directly through CIESIN's file transfer protocol, we did not see a way to download it aggregated to the tract-level. We are therefore unsure whether HSIS were able to directly download tract-level data or whether they aggregated it after downloading (as we did).

the HSIS data, at 254.8 and 68.55. Clearly, then, HSIS are calculating poverty rates over a small subset of tract residents in the CIESIN data. The remainder of Table 5 shows that this problem occurs for every single state in their sample.

Although HSIS clearly have incorrect measures of tract-level poverty in 1990, we have not been able to identify the exact error. One possibility is that because the CIESIN data measure poverty (and other tract-level outcomes) for subgroups – such as age groups – HSIS may have inadvertently computed poverty rates using only one or a subset of groups needed to calculate the overall tract-level measure. This is consistent with them badly undercounting the number of persons both below and above poverty. However, there are two tracts out of 8,705 where the HSIS above-poverty count is higher than in the CIESIN data, and 19 tracts out of 8,705 where the HSIS below-poverty count is greater than in the CIESIN. These exceptions suggest that the difference in estimates across the HSIS and CIESIN data is not due to a dropped cell.[20]

A second possibility is that HSIS downloaded the original data at a level of disaggregation other than the tract, such as the Census place or Census block group level, and made an error when aggregating the data to the tract level. If, for example, some Census block groups were omitted when aggregating the data, then this would explain the largely consistent lower counts of those above and below poverty.[21] However, we do not believe that a data aggregation process produced their error. If there was an error while collapsing the data, the error should not have affected data for Census tracts with only one record number. Restricting the CIESIN to Census tracts that did not change according to the NCDB, and also have only one record per tract, continued to yield poverty rates and counts that do not match HSIS's data. (See Appendix Table A1.)

[20] To investigate this more fully, we also experimented with adding up different subsets of the various population groupings provided in the CIESIN data. We tried a large set of possible population groupings including poverty status by age (P117), sex and age (P118), race and age (P119), Hispanic origin and age (P120), family and presence and age of children (P124), family type and presence and age of children for Hispanic families (P125), family type and age for related children under 18 years (P126), and age of householder by household type (P127), but could not replicate their error. This further suggests that the error was not a dropped cell.

[21] Tract-level data consist of one or more records where each record is identified by a combination of state, year, county, Census tract, Census place, Census block group, county subdivision, Congressional district, and Native American areas.

Third, there could have been an error in translating 1980 to 1990 Census tracts and then 1990 to 2000 Census tracts. However, HSIS would not (or could not) provide the code they used to match tracts across years (the conversion of 1980 and 1990 tracts to 2000 tracts).[22] It also seems unlikely that such a mistake could drive the discrepancies documented in Table 5, since we restrict attention to tracts that, according to the NCDB, did not change between 1980 and 2000. It is still possible that there is an error in matching tracts across time in the different datasets HSIS used, but that seems unlikely since the other tract-level measures match quite well across data sources.

We cannot rule out the possibility that the 1990 data HSIS downloaded from CIESIN were incorrect at the time, but correct now. Nonetheless, perusal of the total persons above and below poverty – which sum to less than 1/10th of the typical tract size – and the sharp increase in poverty rates in 1990 in the data they use, should have raised serious warning flags. Regardless, the important point is not to determine why the data HSIS used were incorrect, but rather to document that the estimates they obtained from these incorrect data dramatically overstate the beneficial effects of state enterprise zones in reducing poverty.

HSIS's results for federal EZs

We would expect similar problems in the estimation of poverty rates and effects of EZs for federally-designated zones.[23] The top panel of Table 6 reports descriptive statistics. Again, across treatment and control tracts, there are large spikes in the 1990 poverty rate in HSIS's data. For example, the poverty rate for Empowerment Zones (EMPZs) in their data goes from 41.76% in 1980, to 62.51% in 1990, and back to 39.15% in 2000. One difference in this case is that there are increases in the 1990 poverty rate in the NCDB data for federally-designated zones (both EMPZs and Enterprise Communities (ENTCs)); for example, from 41.87% in EMPZs in 1980, to 48.35% in 1990, and back to 39.35% in 2000. These increases, however, are nowhere near as large as in HSIS's data.

There is one potential limitation in the data comparing ENTCs to all controls in the state. The

[22] They did give us a contact at a consulting firm who helped them match Census tracts over time, who said he could reconstruct the work if we paid for it.

[23] The data problem seems to be confined to the measurement of poverty, although below we explore other issues in HSIS's estimation of the effects of federal zones. Nonetheless, for brevity we focus on the estimated effects of federal EZs on the poverty rate.

dataset that HSIS's provided for this analysis does not contain tract IDs for tracts not designated as ENTCs (i.e., their controls). Therefore, to combine HSIS's data and the NCDB, we match on outcomes in 2000[24] that appear to have identical values in both datasets.[25] Because this matching process is less than ideal, we are more confident regarding estimates produced using data that match directly on tract IDs. This caveat applies to the last panel of descriptive statistics, and the triple-difference estimates, in the ENTC estimates for the NCDB data, reported in Table 6.

The errors in the measurement of poverty in the HSIS data for 1990 suggest that, again, the HSIS data could generate misleading evidence of the effects of federal EZs on poverty. The bottom panel of Table 6 shows that this is the case for ENTCs; HSIS estimate that ENTC designation reduces the poverty rate by 20.28 percentage points. The NCDB data yield an estimate that is about half as large (an 11.54 percentage point decline). For EMPZs, the erroneous measurement of poverty in HSIS does not create much bias, as both estimates point to a decline in the poverty rate of around 9 percentage points.

Thus, there is less clear evidence of bias from the erroneous measurement of poverty in the 1990 HSIS data for federal EZs. The reason for this is twofold. First, for the nearest and contiguous comparison tracts, the spike in poverty rates in 1990 is almost as large as for the EMPZ tracts, so there is similar bias in the DD estimates for the designated and control tracts, much of which nets out in the DDD estimate. Second, unlike for the state EZ tracts, there is a spike (albeit smaller) in the NCDB data in 1990 poverty rates for the federally-designated tracts.

The spikes in poverty rates in the 1990 NCDB data – which are accurately measured – suggest that, unlike for state EZs, federal zones designated between 1990 and 2000 may have been selected based on particularly bad outcomes in 1990. This is evidenced by the fact that poverty also increased in the nearest and contiguous tracts, which we might expect to have shared outcomes with the tracts actually designated as federal zones. This is reason to be skeptical of the DDD estimates of the effects of federal EZs even

[24] The outcomes (HSIS variable names in parentheses) were state (*state*), county (*county*), number below poverty (*blvpov00*), employment (*employment*), number of households (*numhhld00*), total households (*tothhlds00*), and the number of people with poverty status determined (*abvpov00* + *blvpov00*).
[25] Some tracts are not uniquely identified with these variables because they contain missing values. This resulted in us dropping 219 out of the total 65,442 tracts in the NCDB and 28 out of the total 29,662 tracts in HSIS's All ENTC data.

using the NCDB data, suggesting that another estimation strategy may be needed to obtain reliable estimates of the effects of federal EZs.

IV estimates

HSIS also present IV estimates of the effects of federal EZs, using the strategy described earlier. While an IV approach can potentially address the apparent selection of federal zones based on bad realizations in 1990, we have already explained why we are skeptical of their HSIS's IV strategy. Moreover – and perhaps justifying our skepticism – HSIS's IV estimates strike us as particularly implausible. Their IV estimate of the effect of EMPZ designation on poverty is a 10.73 percentage point reduction in poverty (not significant). Their IV estimate of the effect of ENTC designation is a statistically significant 19.57 percentage point reduction – more than twice as large as for EMPZs.

However, Busso et al. (2013) note that in the round of enterprise zone applications during which these federal zones were created, eight cities received Empowerment Zone designation (became EMPZs), while "49 rejected cities were awarded smaller enterprise communities … [became ENTCs] as consolation prizes" (p. 900, bracketed comment added). ENTCs did not have hiring tax credits; they only received $3 million in Social Services Block Grant (SSBG) funds, and were eligible for tax-exempt bond financing. EMPZs, in contrast, had 20% hiring credits for the first $15,000 in wages earned by each employee who lived and worked in the community, for up to 10 years (declining), and $100 million in SSBG funds. The difference in benefits between EMPZs and ENTCs is so stark that Busso et al. include the rejected zones that became ENTCs in their *controls*. These policy differences, coupled with the fact that HSIS estimate much *larger* effects of ENTCs, provides additional reason to discount HSIS's IV estimates of the effects of federally-designated zones – especially ENTCs. Moreover, in the next sub-section we show that there is essentially no evidence that ENTC designation reduced poverty relative to more appropriate controls.

Matching estimates

In their paper assessing the effectiveness of Empowerment Zones (EMPZs), Busso et al. (2013) assign tracts as controls if they submitted applications in Round I, but were not granted EMPZ status (which, as noted above, often resulted in designation as an ENTC), or if they submitted applications in future rounds. These control tracts are more likely to have shared common outcomes with tracts ultimately awarded EMPZ status, and hence to provide valid counterfactuals. Moreover, we can use the controls *other than* the ENTCs to re-evaluate HSIS's evidence on the effects of ENTCs.

We use data posted by Busso et al.[26] to separately estimate the effects of EMPZ and ENTC designation on poverty, using as control tracts those that applied in Round I but were rejected, applied in future rounds, or both.[27,28] Using Busso et al.'s control tracts produce considerably smaller effects of EMPZ designation on poverty than using HSIS's control tracts with either their data, or the NCDB. The difference is not trivial: Table 7 shows that restricting HSIS's treatment and control tracts to those used by Busso et al. leads to about a 35% smaller effect than the estimate based on the NCDB (−3.46 percentage points smaller, comparing the estimate of −9.60 in the first column of Table 6 to the estimate of −6.20 in the first column of Table 7). The differences are even larger when using all of Busso et al.'s control tracts (instead of the intersection of the Busso et al. and HSIS controls) and including all tracts coded as treated in HSIS (the second and third columns of Table 7). This suggests that, even in the absence of a data coding error (since we are comparing estimates using the NCDB), the control tracts HSIS used do not share the same trends as the treated tracts. Using poorly assigned and perhaps endogenously selected control tracts produces estimates that overstate the effect of EMPZs on poverty alleviation.

Finally, using Busso et al.'s control tracts almost completely eliminates the effect of ENTC

[26] The data can be accessed at https://www.aeaweb.org/articles?id=10.1257/aer.103.2.897 (viewed October 10, 2017).
[27] Federal zones were enacted based on 1990 tract boundaries. Because we use tract boundaries in 2000, some tracts in Busso et al.'s data are only partially treated. We do not code these tracts as treatment or control tracts.
[28] For EMPZs, restricting HSIS's treated and control tracts to the intersection with those in Busso et al., and dropping tracts that had 1980 or 1990 population coded as "0" or "missing" in the NCDB, results in 266 treated tracts and 445 control tracts. For the treatment tracts, there are 280 tracts in HSIS's data that are coded as being awarded Empowerment Zone designation in Round I. Of these, 11 tracts were dropped because of zero 1980 or 1980 population counts in the NCDB, and another three do not appear at all in Busso et al.'s data. For control tracts, HSIS originally coded 14,859 tracts as controls. Busso et al.'s data contain 453 of these tracts (we restrict the Busso et al. data to only include states in HSIS's data), but Busso et al. coded eight of these tracts as receiving EMPZ designation, resulting in 445 control tracts that intersect with HSIS's coding of control tracts. The number of intersecting control tracts is unaffected by dropping tracts with zero populations in 1980 and 1990.

For ENTCs, restricting HSIS's treated tracts to the intersection with those in Busso et al., and dropping tracts that had 1980 or 1990 population coded as "0" or "missing" in the NCDB, results in 355 treated tracts. There are 414 tracts in HSIS's data that are coded as being awarded Enterprise Community designation in Round I. However, only 375 of these tracts have non-zero populations in 1980 and 1990. In Busso et al., 355 of these tracts are coded as treated tracts. The intersection of tracts coded as controls in both Busso et al. and HSIS for the ENTC analysis is 353.

designation on poverty. Table 8 shows that using Busso et al.'s control tracts yield small and mostly statistically insignificant effects of ENTC designation on poverty. Even for the largest effect we estimate (−4.38, in column 1), HSIS's estimate of −20.28 is nearly 500% larger. Moreover, we have less confidence in the estimate in column 1 because, as discussed above, it is based on matching to the NCDB using outcomes in 2000 instead of tract IDs. Conversely, we are more confident in the estimates in columns 2 and 3, which indicate small and insignificant effects on poverty. Regardless, all of our estimates stand in stark contrast to the implausibly large effects that HSIS find, and the evidence is consistent with Busso et al.'s decision to code tracts that received ENTC designation as controls in their analysis of the effects of EMPZs.[29]

We also show the estimates for the other four outcomes HSIS analyze in Appendix Tables A2 and A3.[30] Again, there is no evidence of effects of ENTCs.

IV. Summary and Conclusions

Contrary to most research, Ham et al. (2011, HSIS) claim that state enterprise zones (EZ) generated large poverty reductions. Their conclusions, however, are largely driven by using data with dramatically incorrect measurement of tract-level poverty rates in 1990. This mismeasurement in 1990 plays a crucial role given their estimation strategy, which compares 1990-2000 and 1980-1990 changes in tracts that did or did not receive EZ status in the 1990-2000 period. Using correct data reduces the estimated effect on poverty from a 6.1 percentage point reduction to a 1.25 percentage point reduction – so their estimate was overstated by nearly 500 percent.

HSIS also report very large poverty reductions from federal EZ designation, either as Empowerment Zones (EMPZs), which received substantive benefits including hiring credits, or as Enterprise Communities (ENTCs), which received meager benefits and no hiring credits. In fact, their estimated poverty effects are more than twice as large for the latter – a 20.3 percentage point reduction in poverty for ENTCs, versus 8.8 percentage points for EMPZs.

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[29] Hanson and Rohlin (n.d.) also suggest that ENTCs are natural control tracts for EMPZs.

[30] We report estimates using HSIS's treatment tracts and Busso et al.'s control tracts to focus on evidence using HSIS' treated tracts using preferable controls.

We re-examine these results for federal zones using a matching estimator that is more defensible than the estimators HSIS used. We find that EMPZs did reduce poverty, but the evidence of poverty reductions in ENTCs that HSIS report appears to be entirely spurious.

Thus, after re-evaluating the evidence, we conclude that it is largely consistent with past evidence aside from the HSIS study. State enterprise zones may have had modest effects in reducing poverty (and other research points to no effects). And there may have been more positive effects of federal Empowerment Zones – the narrow set of federal zones, studied by Busso et al. (2013), which received substantial benefits. But the estimates in HSIS dramatically overstate the poverty-reduction effects of enterprise zones. Their estimated effects of state enterprise zones are highly inaccurate because of errors in their data. And their estimated effects of federal zones rely on estimators that are likely invalid, consistently overstate the impacts on poverty, and for one of two types of zones reflect spurious evidence of substantial poverty reductions.

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Table 1: Summary Statistics for State Enterprise Zone Analysis: Comparing Estimates Using NCDB Data and HSIS Data (and Published Results)

	Unemployment rate (%)		Poverty rate (%)		Fraction of households with wage and salary income (%)		Average wage and salary income ($2000)		Employment	
Data	NCDB	HSIS	NCDB	HSIS	NCDB	HSIS	NCDB	HSIS	NCDB	HSIS
ENTZ 1980	7.50***	7.63***	**16.78*** **	**16.41*** **	74.16***	74.39***	39,045***	35,690***	1,718***	1,671***
	(0.36)	(0.37)	(1.51)	(1.44)	(1.03)	(0.96)	(986)	(844)	(69.28)	(64.84)
N	1,175	1,221	1,176	1,245	1,175	1,234	1,175	1,212	1,176	1,264
								[35,626***]		
ENTZ 1990	9.07***	8.87***	**17.79*** **	**25.67*** **	74.34***	74.29***	44,866***	43,301***	1,900***	1,866***
	(0.46)	(0.42)	(1.45)	(1.77)	(0.87)	(0.83)	(1,422)	(1,295)	(71.26)	(65.65)
N	1,175	1,221	1,176	1,245	1,175	1,234	1,175	1,212	1,176	1,264
								[43,306***]		
ENTZ 2000	7.79***	7.72***	**18.33*** **	**17.95*** **	75.00***	75.08***	46,905***	45,759***	1,925***	1,933***
	(0.56)	(0.51)	(1.40)	(1.34)	(0.80)	(0.75)	(1,876)	(1,559)	(76.64)	(70.69)
N	1,175	1,221	1,176	1,245	1,175	1,234	1,175	1,212	1,176	1,264
								[45,820***]		
Nearest NENTZ 1980	6.12***	6.39***	**11.72*** **	**11.81*** **	77.69***	77.23***	44,640***	40,619***	1,798***	1,669***
	(0.48)	(0.47)	(1.33)	(1.21)	(0.93)	(0.84)	(1,204)	(1,246)	(78.81)	(75.89)
N	1,175	1,221	1,176	1,245	1,175	1,234	1,175	1,212	1,176	1,264
		[7.10***]		[12.90***]		[77.44***]		[40,012***]		[1,626***]
Nearest NENTZ 1990	6.64***	6.70***	**11.38*** **	**16.13*** **	76.80***	76.52***	53,124***	50,861***	2,000 ***	1,935***
	(0.40)	(0.38)	(1.52)	(2.24)	(0.77)	(0.62)	(2,973)	(2,666)	(82.91)	(71.93)
N	1,175	1,221	1,176	1,245	1,175	1,234	1,175	1,212	1,176	1,264
		[7.38***]		[19.15***]		[77.17***]		[48,542***]		[1,902***]
Nearest NENTZ 2000	6.22***	6.18***	**12.91*** **	**12.22*** **	75.84***	76.46***	56,377***	55,247***	2,066***	2,061***
	(0.63)	(0.59)	(1.42)	(1.31)	(0.75)	(0.51)	(3,796)	(3,438)	(87.41)	(84.79)
N	1,175	1,221	1,176	1,245	1,175	1,234	1,175	1,212	1,176	1,264
		[6.76***]		[13.92***]		[77.17***]		[52,672***]		[2,004***]
Contiguous NENTZ 1980	5.93***	6.29***	**11.14*** **	**11.46*** **	76.53***	77.45***	44,521***	40,896***	1,803***	1,734***
	(0.45)	(0.47)	(1.33)	(1.20)	(1.26)	(0.86)	(1,301)	(990)	(81.25)	(74.16)
N	1,193	1,227	1,193	1,247	1,193	1,241	1,193	1,261	1,193	1,264
Contiguous NENTZ 1990	6.25***	6.46***	10.80***	15.40***	75.64***	76.98***	53,292***	52,314***	2,025***	2,013***
	(0.34)	(0.34)	(1.52)	(2.14)	(1.18)	(0.61)	(3,012)	(2,690)	(83.97)	(66.68)
N	1,193	1,227	1,193	1,247	1,193	1,241	1,193	1,261	1,193	1,264
Contiguous NENTZ 2000	5.87***	5.96***	11.90***	11.52***	74.90***	76.89***	56,888***	57,279***	2,124***	2,154***
	(0.55)	(0.54)	(1.24)	(1.17)	(0.95)	(0.44)	(3,747)	(3,443)	(85.37)	(76.93)
N	1,193	1,227	1,193	1,247	1,193	1,241	1,193	1,261	1,193	1,264
All NENTZ 1980	6.50***	6.59***	**10.72*** **	**10.77*** **	78.82***	78.56***	48,469***	43,567***	1,567***	1,538***
	(0.22)	(0.21)	(0.54)	(0.50)	(0.65)	(0.61)	(767)	(683)	(42.62)	(40.51)
N	21,922	23,090	21,931	23,420	21,905	23,269	21,905	23,447	21,986	23,488
All NENTZ 1990	6.48***	6.50***	**11.41*** **	**15.77*** **	78.53***	78.26***	55,163***	53,163***	1,918***	1,895***
	(0.28)	(0.27)	(0.56)	(0.71)	(0.56)	(0.53)	(1,193)	(1,146)	(46.51)	(42.95)
N	21,922	23,090	21,931	23,420	21,905	23,269	21,905	23,447	21,986	23,488
All NENTZ 2000	6.46***	6.47***	**12.18*** **	**12.13*** **	78.20***	77.95***	58,520***	57,689***	2,081***	2,073***
	(0.31)	(0.29)	(0.58)	(0.54)	(0.44)	(0.42)	(1,259)	(1,206)	(50.46)	(47.18)
N	21,922	23,090	21,931	23,420	21,905	23,269	21,905	23,447	21,986	23,488

Notes: This table replicates HSIS, Table 1. Columns labeled "NCDB data" attempt to replicate HSIS's estimates using the Neighborhood Change Database (NCDB). Columns labeled HSIS are computed from their data. In a few instances individual estimates reported in the paper differ; these are highlighted in the shaded boxes, and the published estimates reported in square brackets. Each outcome mean is conditioned on not having missing observations for other years for that variable. For example, if there is data for 1990 employment, but there are some missing observations for 1980 employment, the estimate for 1990 will not include those tracts for which 1980 data were missing. Additionally, for the NCDB data, tracts are dropped if they have zero population in 1980 or 1990 (this explains many of the differences in the number of observations between HSIS data and the NCDB data). Standard errors are in parentheses.

Table 2: Double-Difference and Triple-Difference Estimates for State Enterprise Zone Analysis: Comparing Estimates using NCDB and HSIS Data

	Unemployment rate (%)		Poverty rate (%)		Fraction of households with wage and salary income (%)		Average wage and salary income ($2000)		Employment	
Data	NCDB	HSIS	NCDB	HSIS	NCDB	HSIS	NCDB	HSIS	NCDB	HSIS
Double-difference estimates										
E{ENTZ(Δ00)	-0.85***	-0.64**	-0.98*	-3.80***	1.63**	0.84***	-1,214*	-1,928**	-30.19	-59.12
- Nearest NENTZ(Δ00)}	(0.31)	(0.30)	(0.56)	(1.16)	(0.80)	(0.29)	(695)	(955)	(21.57)	(45.68)
N	1,175	1,221	1,176	1,245	1,175	1,234	1,175	1,212	1,176	1,264
								-1,882**		
E{ENTZ(Δ90)	1.05***	0.94***	1.35***	4.94***	1.07**	0.62	-2,664***	-2,631***	-30.17	-71.37**
- Nearest NENTZ(Δ90)}	(0.27)	(0.24)	(0.39)	(1.13)	(0.46)	(0.50)	(984)	(712)	(28.67)	(32.05)
N	1,175	1,221	1,176	1,245	1,175	1,234	1,175	1,212	1,176	1,264
								[-2,585***]		
E{ENTZ(Δ00)	-0.90***	-0.65**	-0.61	-3.81***	1.46*	0.89***	-1,531**	-1,967***	-60.11***	-74.84*
- Contiguous NENTZ(Δ00)}	(0.28)	(0.26)	(0.63)	(1.13)	(0.87)	(0.30)	(627)	(591)	(17.89)	(40.79)
N	1,193	1,227	1,193	1,247	1,193	1,241	1,193	1,261	1,193	1,264
E{ENTZ(Δ90)	1.21***	1.07***	1.40***	5.30***	1.06**	0.37	-3,027***	-3,408***	-49.71*	-83.94***
- Contiguous NENTZ(Δ90)}	(0.27)	(0.23)	(0.40)	(1.06)	(0.49)	(0.49)	(940)	(881)	(27.30)	(30.44)
N	1,193	1,227	1,193	1,247	1,193	1,241	1,193	1,261	1,193	1,264
E{ENTZ(Δ00)}	-0.25	-0.14	-0.12	-4.59***	1.57***	1.47***	-1,867***	-2,129***	-103.1***	-86.67***
- E{All NENTZ(Δ00)}	(0.31)	(0.26)	(0.42)	(0.76)	(0.46)	(0.43)	(805)	(760)	(33.84)	(33.29)
N	23,218	24,465	23,227	24,804	23,202	24,651	23,202	24,834	23,283	24,877
E{ENTZ(Δ90)}	0.97***	0.78***	0.85*	5.37***	1.02**	0.70	-3,707***	-4,192***	-132.2***	-112.2***
- E{All NENTZ(Δ90)}	(0.27)	(0.25)	(0.43)	(0.80)	(0.52)	(0.47)	(591)	(578)	(39.51)	(36.89)
N	23,218	24,465	23,227	24,804	23,202	24,651	23,202	24,834	23,283	24,877
Triple-difference estimates										
Comparison	Contiguous	Contiguous	All	Nearest	Nearest	Contiguous	Nearest	Nearest	All	Contiguous
[E{ENTZ(Δ00) - NENTZ(Δ00)}] - [E{ENTZ(Δ90) - NENTZ(Δ90)}]	-1.88***	-1.64***	**-1.25*** **	**-6.10*** **	0.38	0.45	614.6	703.0*	29.53	68.91**
	(0.25)	(0.23)	(0.32)	(1.21)	(0.52)	(0.30)	(429)	(387)	(21.14)	(32.57)
Observations	1,158	1,227	23,151	1,245	1,153	1,241	1,124	1,212	23,230	1,264
Number of ENTZs	1,158	1,227	1,290	1,245	1,153	1,241	1,124	1,212	1,296	1,264
Number of counties	90	112	317	112	90	112	90	112	317	112

Notes: The double-difference estimates replicate the bottom rows of HSIS, Table 1. The triple-difference estimates replicate HSIS, Table 2. Columns labeled "NCDB data" attempt to replicate HSIS's estimates using the Neighborhood Change Database (NCDB). Columns labeled HSIS are computed from their data. In a few instances individual estimates reported in the paper differ; these are highlighted in the shaded boxes, and the published estimates reported in square brackets. Each outcome mean is conditioned on not having missing observations for other years for that variable. For example, if there is data for 1990 employment, but there are some missing observations for 1980 employment, the estimate for 1990 will not include those tracts for which 1980 data were missing. Additionally, for the NCDB data, tracts are dropped if they have zero population in 1980 or 1990 (this explains many of the differences in the number of observations between HSIS data and the NCDB data). "Nearest" and "Contiguous" estimates are produced by regressing a triple-differenced outcome variable on a constant, with standard errors (in parentheses) clustered at the county level. Estimates using all Census tracts as comparisons ("All") are produced by regressing a double-differenced outcome variable on a dummy variable for zones designated as enterprises and state dummies, with standard errors clustered at the county level. The comparison used is selected by Hausman tests, as explained in the paper. As the bottom panel reports, with the NCDB data the comparison selected was not always the same. However, results were very similar using the comparison from HSIS (results available upon request).

Table 3. Comparing CIESIN and HSIS 1990 Poverty Counts and Rates by State

	Above poverty (count)		Below poverty (count)		Poverty rate	
	CIESIN	HSIS	CIESIN	HSIS	CIESIN	HSIS
All states						
ENTZ 1990	3,297***	257.1***	669.4***	98.45***	17.85***	25.86***
	(108.3)	(10.86)	(49.49)	(8.50)	(1.49)	(1.88)
N	781	781	781	781	781	781
NENTZ 1990	3,437***	267.5***	458.2***	62.00***	12.66***	17.43***
	(108.2)	(9.88)	(25.66)	(4.35)	(0.83)	(1.15)
N	11,600	11,600	11,600	11,600	11,600	11,600
California						
ENTZ 1990	3,381***	285.0***	1,166***	173.8***	25.12***	34.90***
	(207.6)	(39.49)	(156.7)	(33.60)	(1.80)	(1.97)
N	86	86	86	86	86	86
ENTZ 1990	4,063***	335.1***	442.7***	58.72***	9.65***	12.57***
	(104.0)	(15.21)	(30.72)	(7.16)	(0.61)	(1.18)
N	2,181	2,181	2,181	2,181	2,181	2,181
Colorado						
ENTZ 1990	2,196***	163.4**	383.0**	42.25***	15.44**	23.43**
	(236.8)	(34.66)	(78.36)	(6.34)	(4.14)	(5.67)
N	8	8	8	8	8	8
NENTZ 1990	3,114***	258.9***	476.0***	67.10***	14.13***	20.53***
	(156.8)	(21.81)	(44.72)	(7.87)	(1.79)	(3.13)
N	270	270	270	270	270	270
Florida						
ENTZ 1990	2,509***	172.2**	1,748***	298.4***	43.12***	61.28***
	(439.2)	(43.79)	(182.6)	(43.99)	(1.96)	(3.40)
N	17	17	17	17	17	17
NENTZ 1990	3,580***	238.5***	480.6***	59.94***	12.14***	17.71***
	(124.1)	(12.37)	(32.10)	(4.72)	(0.81)	(1.19)
N	1,037	1,037	1,037	1,037	1,037	1,037
Hawaii						
ENTZ 1990	3,691	299.6	974.3	129.9	18.76	28.93*
	(642.8)	(59.88)	(253.5)	(31.10)	(3.73)	(2.86)
N	7	7	7	7	7	7
NENTZ 1990	3,473***	268.9***	313.5***	43.79***	9.74***	11.69***
	(83.56)	(6.59)	(12.88)	(3.18)	(0.84)	(1.01)
N	142	142	142	142	142	142
Illinois						
ENTZ 1990	3,125**	213.3***	466.5*	59.33*	13.51*	22.81*
	(68.00)	(3.22)	(63.50)	(7.11)	(1.95)	(2.75)
N	6	6	6	6	6	6
NENTZ 1990	3,342***	269.0***	504.5***	71.85***	15.42***	21.08***
	(134.1)	(18.70)	(35.48)	(5.43)	(1.38)	(1.64)
N	1,567	1,567	1,567	1,567	1,567	1,567
Massachusetts						
ENTZ 1990	3,431***	255.9***	573.3***	82.75***	15.54***	23.31***
	(160.5)	(16.80)	(61.06)	(7.55)	(1.75)	(2.07)
N	347	347	347	347	347	347
NENTZ 1990	4,382***	350.9***	231.8***	26.87***	5.31***	7.21***
	(129.1)	(7.88)	(14.33)	(3.43)	(0.40)	(0.88)
N	347	347	347	347	347	347

	Above poverty (count)		Below poverty (count)		Poverty rate	
	CIESIN	HSIS	CIESIN	HSIS	CIESIN	HSIS
Nebraska						
ENTZ 1990	-	-	-	-	-	-
	-	-	-	-	-	-
N	-	-	-	-	-	-
NENTZ 1990	3,260***	272.2***	365.7***	48.60***	10.95***	15.26***
	(93.40)	(9.45)	(24.06)	(3.10)	(0.81)	(0.76)
N	91	91	91	91	91	91
New York						
ENTZ 1990	2,306***	190.7***	942.7***	145.9***	31.00***	40.73***
	(254.0)	(22.90)	(89.02)	(16.27)	(3.94)	(4.89)
N	80	80	80	80	80	80
ENTZ 1990	3,090***	228.8***	504.4***	66.45***	14.24***	19.16***
	(236.5)	(17.70)	(77.74)	(12.71)	(2.03)	(2.74)
N	3,246	3,246	3,246	3,246	3,246	3,246
Ohio						
ENTZ 1990	3,682***	297.4***	387.4***	47.42***	10.00***	14.00***
	(130.1)	(15.68)	(49.56)	(6.32)	(1.40)	(2.16)
N	140	140	140	140	140	140
NENTZ 1990	3,149***	246.9***	425.7***	64.97***	13.70***	20.31***
	(142.1)	(13.40)	(16.36)	(3.05)	(0.84)	(1.04)
N	1,324	1,324	1,324	1,324	1,324	1,324
Oregon						
ENTZ 1990	2,979***	251.2***	550.0***	84.81***	15.59***	24.18***
	(123.7)	(13.12)	(51.66)	(13.13)	(1.22)	(3.50)
N	36	36	36	36	36	36
NENTZ 1990	3,389***	269.7***	417.3***	46.39***	11.74***	15.39***
	(212.7)	(23.11)	(27.64)	(2.75)	(1.10)	(1.21)
N	202	202	202	202	202	202
Rhode Island						
ENTZ 1990	4,280***	321.3***	509.3***	69.40**	10.49***	15.88**
	(151.3)	(17.26)	(73.09)	(18.89)	(1.18)	(2.98)
N	15	15	15	15	15	15
NENTZ 1990	3,788***	279.6***	282.5***	31.90***	7.64***	11.41***
	(49.87)	(11.97)	(34.12)	(4.98)	(0.94)	(1.34)
N	100	100	100	100	100	100
Virginia						
ENTZ 1990	2,579***	182.3***	709.1***	105.9***	22.62***	35.73***
	(425.0)	(34.86)	(122.3)	(23.00)	(5.42)	(8.32)
N	17	17	17	17	17	17
NENTZ 1990	3,486***	277.9***	393.0***	45.51***	10.47***	14.11***
	(214.6)	(18.38)	(40.90)	(4.96)	(1.21)	(1.69)
N	642	642	642	642	642	642
Wisconsin						
ENTZ 1990	3,330***	272.6***	548.3***	106.7**	15.35***	27.44***
	(210.1)	(17.41)	(58.26)	(18.93)	(1.35)	(2.81)
N	22	22	22	22	22	22
NENTZ 1990	3,115***	261.7***	461.4***	77.27***	15.12***	22.05***
	(387.9)	(37.21)	(51.16)	(16.14)	(3.45)	(5.34)
N	451	451	451	451	451	451

Notes: Each estimate is generated by regressing the relevant variable on a constant, clustering standard errors (in parentheses) at the county level. Each observation is a tract level measure of poverty. The sample includes only tracts that do not change from 1990-2000, have non-zero 1990 population counts, according to the NCDB. Tracts with missing values for any poverty measure are dropped from the analysis. There are no estimates for ENTZs in Nebraska because there are no ENTZ tracts that meet the sample selection criteria described in this note.

Table 4: Triple-Difference Estimates of Effects on Poverty Rate for State Enterprise Zone Analysis: Comparing Estimates using NCDB and HSIS Data, Triple-Difference Estimates

	Poverty rate (%)	
Data	NCDB	HSIS
Comparison	Nearest	Nearest
EZ x California	-2.31	-7.14**
	(1.47)	(3.61)
EZ x Florida	-2.51	-7.25
	(1.80)	(4.50)
EZ x Massachusetts	-2.07***	-13.95***
	(0.76)	(2.22)
EZ x New York	-3.54**	-8.81***
	(1.39)	(3.36)
EZ x Ohio	0.42	1.91
0	(1.05)	(2.34)
EZ x Oregon	-6.32***	-10.29**
	(2.41)	(4.50)
EZ x Other states	0.62	-1.41
	(1.36)	(2.90)
Observations	1,156	1,245
Number of counties	90	112

Notes: The triple-difference estimates replicate HSIS, Table 2. See notes to Tables 1 and 2 for additional details. For the analyses in this table, the comparison selected with the NCDB data was the same as in HSIS. For the analyses in this table, we exactly replicated the published Ham et. results using their data.

Table 5. Comparing Tract-level Means in CIESIN, NCDB, and HSIS Data, National and by State, 1990

	CIESIN		NCDB		HSIS	
	N	Mean	N	Mean	N	Mean
	All 13 States					
Fraction of HH with wage or salary income	8,675	76.40	8,675	76.40	8,675	76.37
Average wage and salary income ($2,000)	8,675	51,959	8,675	52,981	8,675	51,895
Unemployment rate	8,675	7.74	8,675	7.74	8,675	7.73
Total employed	8,675	1,870	8,675	1,870	8,675	1,863
Poverty rate	8,675	13.91	8,675	13.91	8,675	19.16
Total persons above poverty	8,675	3,339	8,675	3,338	8,675	254.8
Total persons below poverty	8,675	498.0	8,675	498.0	8,675	68.55
	California					
Poverty rate	1,565	10.38	1,565	10.38	1,565	13.47
Total persons above poverty	1,565	3,993	1,565	3,991	1,565	319.7
Total persons below poverty	1,565	474.4	1,565	473.6	1,565	61.67
	Colorado					
Poverty rate	187	15.73	187	15.73	187	23.24
Total persons above poverty	187	2,941	187	2,938	187	225.8
Total persons below poverty	187	505.7	187	505.3	187	70.51
	Florida					
Poverty rate	587	14.35	587	14.34	587	20.81
Total persons above poverty	587	3,549	587	3,549	587	234.6
Total persons below poverty	587	562.1	587	562.1	587	74.01
	Hawaii					
Poverty rate	91	8.83	91	8.99	91	11.66
Total persons above poverty	91	3,472	91	3,482	91	252.7
Total persons below poverty	91	319.3	91	330.1	91	44.41
	Illinois					
Poverty rate	1,145	17.40	1,145	17.40	1,145	23.67
Total persons above poverty	1,145	3,160	1,145	3,160	1,145	249.6
Total persons below poverty	1,145	562.7	1,145	562.7	1,145	81.19
	Massachusetts					
Poverty rate	582	10.63	582	10.62	582	15.63
Total persons above poverty	582	3,885	582	3,885	582	304.4
Total persons below poverty	582	405.6	582	405.5	582	56.60
	Nebraska					
Poverty rate	40	11.38	40	11.38	40	14.97
Total persons above poverty	40	3,245	40	3,245	40	267.9
Total persons below poverty	40	357.3	40	357.3	40	47.77
	New York					
Poverty rate	2,767	15.15	2,767	15.15	2,767	20.44
Total persons above poverty	2,767	3,031	2,767	3,033	2,767	220.3
Total persons below poverty	2,767	540.9	2,767	541.1	2,767	72.20
	Ohio					
Poverty rate	797	14.70	797	14.70	797	21.32
Total persons above poverty	797	3,050	797	3,047	797	240.6
Total persons below poverty	797	435.0	797	434.9	797	66.63
	Oregon					
Poverty rate	152	13.20	152	13.20	152	17.14
Total persons above poverty	152	3,309	152	3,309	152	261.7
Total persons below poverty	152	475.2	152	475.2	152	53.06
	Rhode Island					
Poverty rate	91	8.48	91	8.48	91	13.14
Total persons above poverty	91	3,824	91	3,817	91	277.7
Total persons below poverty	91	329.0	91	328.7	91	40.09
	Virginia					
Poverty rate	330	10.09	330	10.10	330	14.16
Total persons above poverty	330	3,280	330	3,277	330	254.2
Total persons below poverty	330	339.5	330	339.5	330	45.05
	Wisconsin					
Poverty rate	341	17.33	341	17.33	341	25.43
Total persons above poverty	341	2,942	341	2,943	341	243.3
Total persons below poverty	341	505.5	341	505.6	341	88.61

Notes: Includes only tracts whose boundaries did not change from 1980-2000, based on the NCDB.

Table 6: Summary Statistics and Triple-Differences Estimates for Federal Enterprise Zone Analysis: Comparing Estimated Effects on Poverty Rates Using NCDB Data and HSIS Data (and Published Results)

	Empowerment Zones (EMPZ)		Enterprise Communities (ENTC)	
Data	NCDB	HSIS	NCDB	HSIS
Descriptive statistics				
EZ 1980	41.87***	41.76***	32.77***	32.13***
	(1.16)	(1.11)	(1.13)	(1.19)
N	264	267	342	340
EZ 1990	48.35***	62.51***	40.03***	55.69***
	(1.53)	(2.22)	(0.99)	(1.70)
N	264	267	342	340
EZ 2000	39.35***	39.15***	34.82***	35.04***
	(0.88)	(0.88)	(1.20)	(1.16)
N	264	267	342	340
Nearest NENTZ 1980	36.28***	36.41***	22.48***	22.27***
	(1.55)	(1.46)	(0.91)	(0.89)
N	264	267	342	340
		[35.69***]		[21.43***]
Nearest NENTZ 1990	38.77***	53.21***	24.16***	35.68***
	(1.55)	(1.56)	(1.34)	(2.05)
N	264	267	342	340
		[53.60***]		[34.51***]
Nearest NENTZ 2000	35.27***	35.12***	25.01***	24.91***
	(1.16)	(1.15)	(1.28)	(1.23)
N	264	267	342	340
		[34.84***]		24.09***]
Contiguous NENTZ 1980	35.44***	35.65***	21.25***	20.92***
	(1.46)	(1.36)	(0.85)	(0.82)
N	264	268	343	346
Contiguous NENTZ 1990	38.10***	52.85***	23.00***	33.73***
	(1.42)	(1.51)	(1.24)	(1.74)
N	264	268	343	346
Contiguous NENTZ 2000	34.88***	34.90***	23.55***	23.11***
	(1.12)	(1.07)	(1.15)	(1.11)
N	264	268	343	346
All NENTZ 1980	11.04***	11.06***	9.75***	9.90***
	(0.84)	(0.78)	(0.29)	(0.27)
N	13,907	14,745	27,146	28,208
All NENTZ 1990	11.83***	16.64***	10.89***	15.45***
	(0.87)	(1.12)	(0.35)	(0.46)
N	13,907	14,745	27,146	28,208
All NENTZ 2000	12.22***	12.21***	11.07***	11.11***
	(0.87)	(0.81)	(0.38)	(0.35)
N	13,907	14,745	27,146	28,208
Triple-difference estimates				
	Contiguous	Contiguous	All	Contiguous
[E{EZ(Δ00) - NENTZ(Δ00)}] -	-9.60***	-8.81***	-11.54***	-20.28***
[E{EZ(Δ90) - NENTZ(Δ90)}]	(1.84)	(2.78)	(0.53)	(2.29)
Observations	264	268	27,520	346
Number of EMPZs	264	268	374	346
Number of counties	9	14	533	57

Notes: See notes to Tables 1 and 2. In the first two columns, the numbers of counties differ because there are four counties in the HSIS data that have only one EMPZ, and one county has two EMPZs. All five of these counties have zero populations in 1980 in the NCDB and are therefore not used for the NCDB estimates.

Table 7. Estimated Effects of Empowerment Zone Designation on Poverty Rates Using Matched Controls from Busso et al. (2013) and NCDB Data

	Poverty rate (%)	Poverty rate (%)	Poverty rate (%)
EMPZ	-6.20***	-5.95***	-5.88***
	(2.17)	(2.23)	(2.20)
Treatment tracts	HSIS ∩ BK	HSIS ∩ BK	HSIS
Control tracts	HSIS ∩ BK	BK	BK
Observations	707	950	952
Number of counties	30	36	37

Notes: Estimates generated using the NCDB and limited to tracts with non-zero populations in 1980 and 1990. Treated tracts denoted “HSIS & BK” are those coded as being assigned Round I Empowerment Zone (EMPZ) status in both HSIS’s and Busso et al.’s data. Treated tracts denoted “HSIS” are only those coded as being assigned Round I EMPZ status in HSIS’s data. Control tracts denoted “HSIS & BK” are the intersection of those coded as NEMPZ tracts in HSIS’s data and those in Busso et al. that are identified as having applied for Round I EMPZ status but were not awarded EMPZ designation (many of which received ENTC designation), those that applied in future rounds, or both. Control tracts denoted “BK” are only those coded as non-EMPZ tracts in Busso et al. that are identified as having applied for Round I EMPZ status but were not awarded EMPZ designation (many of which received ENTC designation), those that applied in future rounds, or both.

Table 8. Estimated Effects of Enterprise Community Designation on Poverty Rates Using Matched Controls from Busso et al. (2013) and NCDB Data

	Poverty rate (%)	Poverty rate (%)	Poverty rate (%)
ENTC	-4.38*	-2.91	-2.94
	(2.53)	(1.95)	(1.84)
Treatment tracts	HSIS ∩ BK	HSIS ∩ BK	HSIS
Control tracts	HSIS ∩ BK	BK	BK
Observations	718	1,410	1,426
Number of counties	66	84	90

Notes: Estimates generated using the NCDB and limited to tracts with non-zero populations in 1980 and 1990. Treated tracts denoted “HSIS & BK” are those coded as being assigned Round I Enterprise Community (ENTC) status in both HSIS’s and Busso et al.’s data. Treated tracts denoted “HSIS” are those coded as being assigned Round I ENTC status in HSIS’s data. Control tracts denoted “HSIS & BK” are intersection of those coded as NENTC tracts in HSIS’s data and those in Busso et al. that are identified as having applied for Round I EMPZ status but were not awarded EMPZ, ENTC, or Enhanced Enterprise Community designation those that applied in future rounds, or both. Control tracts denoted “BK” are those in Busso et al. that are identified as having applied for Round I EMPZ status but were not awarded designation as either EMPZs, ENTCs, or Enhanced Enterprise Communities, those that applied in future rounds, or both.

Appendix Table A1. Comparing CIESIN, NCDB, and HSIS Data in 1990 for tracts with only one record in the CIESIN

	CIESIN		NCDB		HSIS	
	N	Mean	N	Mean	N	Mean
			All 13 States			
Fraction of HH with wage or salary income	697	76.19	697	76.18	697	76.07
Average wage and salary income	697	52,830	697	53,864	697	52,791
Unemployment rate	697	8.87	697	8.87	697	8.85
Total employed	697	1,014	697	1,013	697	1,009
Poverty rate	697	16.48	697	16.48	697	20.97
Total persons above poverty	697	1,741	697	1,740	697	123.3
Total persons below poverty	697	322.5	697	322.6	697	44.25

Notes: Includes only non-changing tracts, based on the NCDB. Sample is conditioned on all three data sets having non-missing observations for all outcomes.

Appendix Table A2. Estimated Effects of Empowerment Zone Designation on Other Outcomes Using Matched Controls from Busso et al. (2013) and NCDB Data

	Unemployment Rate (%)	Fraction of Households with Wage and Salary Income (%)	Average Wage and Salary Income ($2000)	Employment
EMPZ	-4.49***	4.23**	1,746	89.49***
	(1.46)	(1.92)	(1,451)	(33.84)
Treatment tracts	HSIS	HSIS	HSIS	HSIS
Control tracts	BK	BK	BK	BK
Observations	948	950	950	959
Number of counties	37	37	37	37

Notes: Estimates generated using the NCDB. Treated tracts those coded as being assigned Round I Empowerment Zone (EMPZ) status in HSIS's data. Control tracts denoted "BK" are only those coded as non-EMPZ tracts in Busso et al. that are identified as having applied for Round I EMPZ status but were not awarded EMPZ designation (many of which received ENTC designation), those that applied in future rounds, or both.

Appendix Table A3. Estimated Effects of Enterprise Community Designation on Other Outcomes Using Matched Controls from Busso et al. (2013) and NCDB Data

	Unemployment Rate (%)	Fraction of Households with Wage and Salary Income (%)	Average Wage and Salary Income ($2000)	Employment
ENTC	1.65	1.52	-85.54	54.84
	(1.05)	(1.24)	(1,179)	(42.58)
Treatment tracts	HSIS	HSIS	HSIS	HSIS
Control tracts	BK	BK	BK	BK
Observations	1,426	1,422	1,422	1,433
Number of counties	90	90	90	90

Notes: Estimates generated using the NCDB. Treated tracts denoted "HSIS & BK" are those coded as being assigned Round I Enterprise Community (ENTC) status in both HSIS's and Busso et al.'s data. Treated tracts denoted "HSIS" are those coded as being assigned Round I ENTC status in HSIS's data. Control tracts denoted "BK" are those in Busso et al. that are identified as having applied for Round I EMPZ status but were not awarded designation as either EMPZs, ENTCs, or Enhanced Enterprise Communities, those that applied in future rounds, or both. We were unable to use HSIS's ENTC control tracts because the tract identifiers were missing in their data for tracts not awarded ENTC designation (to be clear, the tract identifiers are populated for tracts they code as being awarded ENTC).